Experimental Science Versus Philosophical Matter Theory in the Work of Robert Boyle

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It is important to distinguish between the claims embodied in seventeenth-century matter theories of the kind defended by the mechanical philosophers and the new experimental science. The former were general theories of the ultimate structure of matter defended mainly by appeal to notions ofintelligibility, or some such thing. The latter involved localised claims about specific phenomena that were defended by experiment. Insofar as the matter theories had empirical support it was of a very weak kind. They were accommodated to the phenomena rather than being confirmed by the phenomena in the way that experimental knowledge could be and often was.

The philosophical distinctions, between ultimate matter theories and items of experimental knowledge and between accommodation to and confirmation by the phenomena, are important for an adequate understanding of what was scientific about the scientific revolution of the seventeenth century. In this paper I defend this claim, and defend myself against the charge that I am imposing contemporary distinctions on the past, in the context of the work of Robert Boyle.

Boyle in fact made explicit and to some extent put into practice versions of the distinctions introduced above. He invoked a scale of causes and explanations ranked according to their degree of empirical accessibility. He distinguished between 'intermediate' causes and explanations accessible to experimental exploration and ultimate mechanical causes that were remote from what could be accessed empirically. Examples of the former are the weightand spring of the air invoked by Boyle to explain a range of pneumatic phenomena and which could be explored by experiments such as those involving the air pump. The latter involved causes and explanations that invoke the shapes, sizes and motions of portions of impenetrable matter that conformed to the dictates of Boyle's mechanical matter theory.

On the face of it, Boyle's distinction between empirically accessible knowledge of intermediate causes and knowledge of remote mechanical causes is difficultto reconcile with his oft-repeated claim that his mechanical matter theory had empirical support. The problem is removed once it is recognised that the kind of support Boyle considered his mechanical theory to have had was of a weaker kind than the stringent kind of experimental support he demanded of his experimental claims. Mechanical matter theory, according to Boyle, is supported by experimental knowledge of a phenomenon to the extent that a mechanism conforming to the dictates of his matter theory canbe contrived that would be capable of reproducing that phenomenon. Arguments of this kind are especially effective if the phenomenon accommodated poses problems for rival matter theories (such as those involving immutable substantial forms, for example). There is no requirement here that there be evidence that the contrived mechanism is the one actually existing in nature. Boyle illustrated with an analogy. Mystery about the regular motion of the hands of a watch can be removed by proposing a mechanism that would be capable of reproducing that motion. The proposed mechanism is

unlikely to conform to the actual one, but at least its construction removes the need to assume the watch is animated.

I have three observations to make about this kind of support. Firstly, it is a very weak kind of support. Boyle inadvertently highlighted this fact when he stressed the flexibility of the mechanical philosophy stemming from the very wide variety of shapes, sizes and motions that can be attributed to the ultimate mechanical particles. Whatever this degree of flexibility, it was not sufficient to undermine my second point, namely, that there were common phenomena that Boyle could not accommodate in his matter theory. Leaving aside chemical and biological phenomena that posed difficulties, the spring and weight of air involved in Boyle's pneumatics had proved resistant to attempts to explain them mechanically as Boyle freely and explicitly acknowledged. My third point is that, even if it is admitted that mechanical matter theory had empirical support in some sense, that sense differed from the strong kind of support that Boyle required of experimental knowledge. As a consequence, my insistence on the significance of the distinction between the status and character of mechanical matter theory and experimental knowledge such as Boyle's pneumatics stands.

While Boyle refrained from an attempt to contrive mechanisms capable of accounting for pneumatics, he showed no such restraint in chemistry. That area was the main one in which Boyle sought for a fruitful relationship between experimental science and fundamental matter theory. I maintain that Boyle's efforts in this regard did not inform his experimental chemistry in a productive way. In keeping with the general structure of his accommodations of phenomena to mechanical matter theory, Boyle sought mechanisms capable of accounting for chemical phenomena known by other means. The contrived character and diverse nature of the range of mechanisms invoked by Boyle to accommodate chemistry attests to the fact that he lacked, and understandably and necessarily lacked, any theory at the mechanical or corpuscular level capable of guiding or otherwise engaging with experiment. Whatever success Boyle had as a chemist, it did not stem from his mechanical matter theory and offered no significant support for it. These claims are based on a detailed analysis of Boyle's attempts to contrive mechanisms to explain chemistry, especially those in 'The Mechanical Origin of Qualities' (1675), an essaywhich has not been given the attention it deserves. In that work Boyle can be found contriving more than one possible mechanism capable of explaining a specified chemical phenomenon. If accommodation is the aim, then two accommodations are better than one. By contrast, mutually incompatible explanations were not acceptable in Boyle's experimental science. He modified and added to his experiments in pneumatics to rule out explanations that were alternatives to his own, for instance.

My account and appraisal of Boyle's chemistry conflicts with one recently advocated by William Newman. Both he and another Boyle scholar, Peter Anstey, have published criticisms of an earlier published version of my distinction between Boyle's mechanical philosophy and his experimental science. The papers that these two scholars are to submit to &HPS2, combined with mine, should make for an interesting and productive Symposium should all three be accepted.